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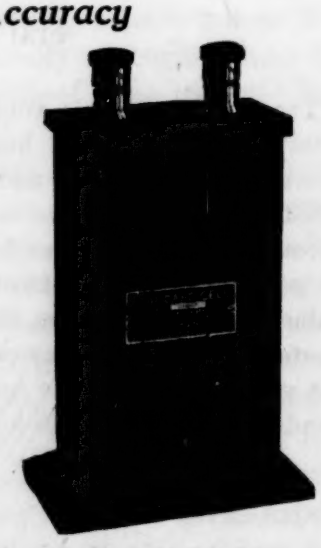

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THE STATISTICAL SIGNIFICANCE OF EXPERIMENTAL DATA[1]

WHEN a few days ago your secretary, Mr. W. T. Bovie, acting on pressure from your chairman, Mr. J. S. Hughes, urged me with their well-known energies to speak in this symposium they left me little chance to refuse. As I understand the circumstances I am a sort of "pinch hitter" for Mr. J. Arthur Harris, whose long-continued biometric studies would clearly indicate him for this place, but whose absence in the south made it necessary to find a substitute. From him you might reasonably have expected a home run; you must be content with me if I bunt out a one-bagger just to keep the game going.

I should have liked to have more time for preparation. The literature upon the statistical aspects of feeding experiments is not microscopic and the data available for statistical study are extensive. Moreover, my Yale training received here twenty odd years ago under J. Willard Gibbs was not such as to make comfortable for me the presentation of somewhat hastily collected notes. There was not in those days the fervid impatience in science that has developed in recent times in some quarters, and Gibbs himself was a model to any young man not only in his scientific thinking but in his modest and painstaking contemplation of some of the most intricate problems of nature—statistical problems. It may not be amiss if I quote these words from the preface of his last great work entitled "Elementary Principles in Statistical Mechanics" written in 1901:

> We avoid the greatest difficulties when giving up the attempt to frame hypotheses concerning the constitution of material bodies, we pursue statistical inquiries as a branch of rational mechanics. In the present state of science, it seems hardly possible to frame a dynamic theory of molecular action which shall embrace the phenomena of thermodynamics, of radiation, and of the electrical manifestations which accompany the union of atoms. Yet any theory is obviously inadequate which does not take account of all these phenomena. Even if we confine our attention to the phenomena distinctly thermodynamic, we do not escape difficulties in as simple a matter as the number of degrees of freedom of a

[1] An address prepared by request as part of a symposium on feeding experiments held by the Biochemical Section of the American Chemical Society, meeting in New Haven during the week of April 2–7 in connection with the dedication of the new Sterling Chemical Laboratory of Yale University.

diatomic gas. . . . Certainly, one is building on an insecure foundation who rests his work on hypotheses concerning the constitution of matter.

Difficulties of this kind have deterred the author from attempting to explain the mysteries of nature, and have forced him to be contented with the more modest aim of deducing some of the more obvious propositions relating to the statistical branch of mechanics. Here, there can be no mistake in regard to the agreement of the hypotheses with the facts of nature, for nothing is assumed in that respect. The only error into which one can fall is the want of agreement between the premises and the conclusions, and this, with care, one may hope, in the main, to avoid.

How very antiquated this sounds to-day when for much of the past two decades the members of the American Chemical Society have been listening as they have this very week to numerous attempts to frame hypotheses concerning the constitution of material bodies, however insecure a foundation such hypotheses may have appeared to Gibbs. The mysteries of nature which Gibbs left somewhat to one side as beyond his modest aim were, however, the mysteries of inorganic nature. Yet in this section of your society we face the vaster difficulties of organic nature, of living matter, and in this symposium the mysteries of *nurture*. It is doubtful if we may hope even with all due care to avoid, in the main, a falling into error. We are confronted with variability of feeds, however carefully we may try to uniformize them, with variation in the experimental animals, however so carefully we select them, with inadequacy of statistical material, no matter how diligently we collect the data. It is necessary to bring to bear every possible check, to exercise all conceivable care in judgment, and yet withal to be modest in our conclusions.

One check, one basis for judgment which we have to-day we did not have in readily available form a quarter century ago. The statistical method in biometrics was just then gaining headway at the hands of Pearson and his incipient school. I remember very clearly with what interest and instruction I read those early papers of Pearson while studying the statistical method with a brilliant young Yale economist in the years 1899–1902. They seemed to open up large possibilities for a scientific basis of political economy quite different from that other scientific method followed by Walras, by Pareto and by Irving Fisher in one of his notable early papers. On the whole, however, it is my judgment that up to the present time it has been not the economist but the student of biology who has most availed himself of these newer statistical methods; it would be interesting to ponder the reason why.

I have been asked to define certain terms and illustrate certain calculations employed in the statistical treatment of experimental data, and this I shall presently do. But in the first place I wish to make some general comments on the philosophy of the statistical interpretation of experimental data. A method is a dangerous thing unless its underlying philosophy is understood, and none more dangerous than the statistical. Our aim should be, with care, to avoid, in the main, erroneous conclusions. In a mathematical or strictly logical discipline the care is one of technique; but in a natural science and in statistics the care must extend not only over the technique but to the matter of judgment, as is necessarily the case in coming to conclusions upon any problem of real life where the complications are great. Over-attention to technique may actually blind one to the dangers that lurk about on every side—like the gambler who ruins himself with his system carefully elaborated to beat the game. In the long run it is only clear thinking, experienced feeling and a patient poise, not automatic systems and methods, that win the strongholds of science—witness the lives and works of those founders of two branches of chemistry of prime importance to this biochemical section: Gibbs in physical chemistry and Pasteur in the chemistry of life.

If you undertake to measure a room for wallpaper, or a court for tennis, you take some simple measuring device and proceed. You determine the measures needed. You may repeat the work as a check but not for the purpose of averaging the results. There is nothing statistical in your mind. The same is true for all our ordinary weights and measures; we weigh and measure; we may check or get somebody else to check the result, we do not average. When a finer or more accurate measure is needed we have recourse to a better or more sensitive instrument; if none can be had with sufficient sensitivity we may even resort to devising one. Generalizations are unsafe, but I will venture the guess that it is always our ideal to have at hand instruments that will enable us directly to read the measures desired and thus spare us the statistical method of analysis. The history of the development of physics has been constantly attended by the conflict between the more accurate instrument and the call for the ever more precise determination of physical properties of matter.

But the time seems always to come when the necessary precision transcends the available appliance. This time came early in the refined measures of astronomy, and already a hundred years ago the treatment of astronomical data was statistical. The most careful measures were most carefully and patiently repeated and the resulting mass of material was reduced by the method of averages and of least squares. The errors or departures from the mean were all small, for the simple reason that the observational

methods and the objects observed were of such a nature that a high degree of precision was directly attainable. Such small deviations as remained to be reduced by statistical treatment were due to a large number of forces or causes each of which if operating alone might produce a considerable irregular and asymmetric variation in the observations, but which were balanced in such a way that the actual deviations were not only small but had a high degree of symmetry and lawfulness. It was under such circumstances that the so-called normal law of errors, proposed and discussed by Laplace about the time that Gauss was learning to walk, came into general use. The law is often called Gauss's law and is figured geometrically as you all know by the bell-shaped probability curve (Fig. 1).

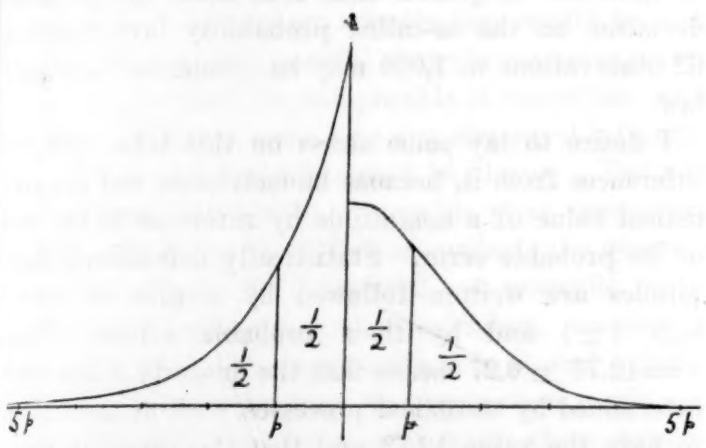

FIG. 1. On the left is Laplace's frequency curve; on the right is Gauss's. The curves are here plotted to a scale which makes the actual probable error p the same; the areas under the two curves are equal and each is divided into two equal parts by the vertical ordinate at p. Each curve should be reproduced symmetrically on the other side of the central vertical line, but the drawing is clearer when only one half of each curve is shown.

During the past century and a fraction a large number of alleged proofs of the normal law of errors or deviations from the arithmetic mean have been proposed and a variety of theoretical objections to it have been propounded. Bertrand brushes aside alike proof and objection with the statement: "Gauss's formula should be adopted. Observation confirms it; that is sufficient for its applications. Its consequences minutely examined are always found accordant with the facts." He is writing, be it clearly understood, from the viewpoint of the astronomer or physicist engaged in precise measurements many times repeated. Indeed, under these conditions we must even recognize the validity of theoretical criteria which may be applied to any such set of data to determine whether or not they are bona fide or have been retouched. Moreover, it is astonishing to find by experience how few observations will serve as a large number. I well remember an experiment I performed with a mirror galvanometer. Some 15 readings were taken to establish a mean. As is always the case, the galvanometer was moving somewhat erratically, due to a large number of conflicting forces—air currents, changes of temperature, electric cars passing in the distance, etc.—despite an effort carefully to shield the vitals of the instrument from external influences. Some of the readings were seemingly unduly far from the mean, and yet if they were discarded the results would not so well accord with the theoretical checks and would tell the tale of presumptive experimental dishonesty. It would have been interesting to apply such criteria to Dr. Cook's famous polar observations; it would take a deal of trouble so to fake the observations as to avoid detection.

So excellent were the results of Gauss's law that many came to believe that it was of wider applicability than either its philosophical premises or its experimental verifications warranted. One of the triumphs of modern statistical theory has been the return to the more general consideration of Laplace and our emancipation from the tyranny of a law too restricted to serve in biometrics. True, there are some fields in which a close observance of the normal law is revealed, as in the distribution of heights among the members of a large population. But little as we know about the true cause of the specific height of any one of us we do know that many major, more or less independent, causes are at work, that these are balanced effectively about a mean from which the deviations are reasonably small. There is no obvious reason why the distribution of Gauss should not apply.

The matter is very different if we study the length of our lives instead of that of our bodies. The frequency distribution of the ages at death among a large population is a reasonably definite affair of undoubted major biological significance; but the curve is not symmetric and has little similarity to that of Gauss. According to the American Experience Mortality Table starting with 100,000 persons alive at the age of 10, the number of deaths per annum is about 750 the first year, decreases slowly to about 720 at the age of 27, increases to about 1,000 at 50 years, and then rapidly to a maximum of 2,500 at the age of 73, from which it falls rapidly to zero in the next 25 years. Now the length of our life surely depends, like the length of our bodies, upon a large variety of conflicting causes, but they do not produce a symmetrical balance about a mean nor are the deviations from the mean small in the sense that they are in precise physical observations or even in the case of our stature. The law of Gauss is plainly contra-indicated.

Although many of the frequency distributions of life are asymmetric and although the general theory

of curve fitting is of importance in biometry and will in due time become important even for such elementary matters as the statistical study of data resulting from feeding experiments, we shall restrict ourselves here to symmetrical laws wherein the chances of positive deviations are equal to the chances of negative deviations of the same magnitude. And of the symmetric laws other than Gauss's one of the most interesting is the first law of errors or small deviations ever proposed. It was put forward by Laplace in 1774 four years before he suggested the normal law. Let us measure deviations not from the arithmetic mean but from the median, that is, from that datum which stands in the middle of a series of observations arranged according to magnitude. The median or middle datum is much used in economic studies instead of the arithmetic mean and in many ways is simpler to use. Now if d be the deviation from the median, Laplace's suggestion is that the frequency of occurrence of a deviation of magnitude d is proportional to

$$e^{-kd} = \frac{1}{(2.718\ldots)^{kd}}$$

In the figure we have on the right the normal law and on the left the law of Laplace. It should be observed that each of the curves should be reproduced symmetrically on the other side of the vertical line, because both are symmetric; but this would complicate the figure. Note that both laws make small deviations much more frequent than large ones, as is ordinarily the case in symmetrical laws. But the Laplace law begins to fall off rapidly and falls off steadily more slowly; whereas Gauss's begins by falling off slowly, then falls faster and finally falls slowly again. The relative numbers of deviations which lie between definite limits are measured by the area under the curve between verticals; and if a vertical be drawn at such a point that the whole area to one side of the middle line is cut exactly in two the deviation represented by that position is called the *probable error* or *probable deviation* p. This is a technical term which in common parlance means merely that the betting is even as to whether a deviation will be greater or will be less than the probable deviation—that in the long run there will be as many deviations larger as there are smaller than this. The curves have been drawn in such a manner that the probable deviations are equal.

As the curves are constantly falling off with increasing deviations, the areas under the curves, toward the right or toward the left as the case may be, are also diminishing and the chances of really large errors are very small. It is customary to take as the unit of reference the probable deviation. The following table shows the chance of deviations being greater than 1, 2, 3, 4 or 5 times the probable value p.

TABLE I

Chances of a deviation greater than 1, 2, 3, 4, 5 times the probable

Laplace's Law	Gauss's Law	Tchebycheff's Criterion
$1p$ .5000	.5000	.....
$2p$ .2500	.1773	less than 0.550
$3p$ .1250	.0430	" " 0.233
$4p$ .0625	.0070	" " 0.137
$5p$ .0312	.0007	" " 0.088

This table shows how fast the chances of large deviations diminish; it shows further how much faster they diminish under the normal law (Gauss) than under Laplace's first law. Only about seven observations in 1,000 can be greater than four times the probable deviation on the so-called probability law; whereas 62 observations in 1,000 may be greater on Laplace's law.

I desire to lay some stress on this table and the inferences from it, because biometricians test the statistical value of a magnitude by reference to the size of its probable error. Statistically determined magnitudes are written followed by a plus or minus sign ($\pm$) and by their probable errors. Thus $x = 12.73 \pm 0.27$ means that the quantity x has been determined by statistical processes, such as averaging, to have the value 12.73 and that the probable error of the determination is 0.27. Or it is even betting that the true value of x lies between $12.73 - 0.27 = 12.46$ and $12.73 + 0.27 = 13.00$. If now we have evidence that the law of the frequency of the distributions is Laplace's second law (the law of Gauss), we may go further and say that the chances are only 43 in 1,000 that the quantity x lies outside the limits defined by thrice the probable error:

$$12.73 - 3 \times (0.27) = 11.92 \text{ and } 12.73 + 3 \times (0.27) = 13.54.$$

Odds of $1{,}000 - 43 = 957$ to 43 or better than 22 to 1 are so high that they represent a reasonable degree of certitude and consequently, when the probable error of a magnitude is less, particularly when it is much less, than one third of the magnitude itself we conclude that the magnitude is statistically significant.

Such a conclusion for its cogency depends tacitly on the fact, if it be a fact, that the chances of large deviations fall off very rapidly with the increase in the deviation as is the case with Gauss's law. On Laplace's law there is not merely one chance in 23, there is one chance in eight that an error exceeds thrice the probable error. If the frequency distribution is unknown, the chances of large deviations are likewise unknown and there is no safe theoretical ground on which those chances can be estimated by

the values set down under Gauss's law in the table. There is a theorem due to the famous Russian Tchebycheff which states that the error in an average value does not exceed a multiple mp of the probable error oftener than once in $2.2/m^2$ times. In the table I have inserted Tchebycheff's Criterion to show how pessimistic he is as to the certitude of statistical inferences in comparison with Gauss. Why is it, then, that biometricians, who deal with material often scanty and of great complexity and diversity of law and with errors neither small nor symmetric, place such confidence in the probable error that a deviation of thrice the probable is regarded as almost impossible and a deviation of four times the probable as quite impossible?

Adequately to answer this question would require an elaborate behavioristic study of biometricians, and I fear that as laboratory animals they would be such varied and variable material that the probable errors of the results would be comparable in magnitude with the results themselves, so that any statistical inference in answer to the question would be illusory. But you can not treat a scientific man or his work exclusively or even largely by the statistical method; the question of judgment must be considered. A scientific investigator, particularly one of the leaders, develops a feeling for his work, an experience in it, and a judgment often so sound that his conclusions merit our most respectful confidence even when those conclusions are apparently founded on very little else than the investigator's intuition. The mature "hunch" of a genius is better than many a scientific demonstration. And it is undoubtedly the experience and belief of many statisticians of the first water that somewhere between three and four times the probable error comes the safe point in drawing conclusions. I mean the safe point for them.

Let me tell you a story of the great astronomer William Herschel. He desired to know the direction of the sun's motion in space. This is a statistical problem; for by the motion of the sun in space is meant its motion relative to the rest of the stars, of which there are many millions visible to our telescopes. Now Herschel had a keen personal acquaintance with the stars and selected with great judgment just seven with respect to which he would determine the solar motion. It would be a rash young statistician who would maintain that seven stars were enough to constitute a fair sample of the sidereal universe for any statistical purpose. Yet Herschel determined the direction of the solar motion with what has proved to be marvelous exactness. We may allow something for luck, but we must not too much discount the efficacy of a Herschel's intuition in the selection of material.

I must now come to some methods of statistics. For illustrative material I shall use certain data supplied to me for the purpose by one of your members from his experiments on feeding pens of guinea pigs raw and variously processed milk in an effort to determine the influence of various methods of treating on the vitamin C content of milk. There were a great many trials involving 8 or 12 pigs each and 144 pigs in all. Let me take the data on boiled milk from one run. There were eight pigs. The first column gives their weight at the start, the second the number of days before scurvy developed. The numbers in the second column are added together and divided by the number (8) of pigs to find the mean length of time (18.75 days) before scurvy developed on the boiled milk ration. The third column gives the deviations of the individual numbers of days from this mean, and the fourth column the squares of these deviations. Means are found also for these two columns.

TABLE II

Weight at start	Days to scurvy	Deviation from mean	Deviation squared	Statistical constants
222	19	0.25	0.0625	$M = 18.75$
205	17	1.75	3.0625	$\delta = 0.875$
185	17	1.75	3.0625	$\sigma = 1.199$
185	19	0.25	0.0625	$p = 0.809$
190	19	0.25	0.0625	$E_m = 0.286$
370	21	2.25	5.0625	$E\sigma = 0.202$
335	19	0.25	0.0625	
195	19	0.25	0.0625	
Sum	150	7.00	11.5000	
Mean	18.75	0.875	1.4375	

The mean deviation is $\delta = 0.875$ days, the mean square deviation is $\sigma^2 = 1.4375$. The square root of the mean square deviation is called the *standard deviation*; in this case its value is $\sigma = 1.199$. The probable deviation has been defined geometrically on the (symmetric) frequency curve; it has also been defined for that case as that deviation than which the greater are equally numerous with the lesser deviations in the long run. As nothing is known relative to the ideal frequency distribution in this experiment other than the information given by the data themselves we must fall back on a formal arithmetic definition which is as follows: The probable deviation is $p = 0.6745\,\sigma$. This is merely the numerical relation between the probable deviation p and the standard error σ when the observations are infinitely numerous and distributed according to Gauss's law. If the true law of distribution of the data were not Gaussian the value $p = 0.6745\,\sigma$ might have small relation to the true probable error. For example, if the true law were Laplacean, the actual probable error would be not $0.6745\,\sigma$ but only $0.49\,\sigma$; the use of the arithmetic definition would give a probable error too large which

fortunately would be on the safe side (the figures in the first column of Table 1 would be 0.387, 0.150, 0.058, 0.022, 0.009, approximately, instead of the value given, but even these are far larger than the figures in column 2). The value of the individual observations may be written

$$\text{days} = 18.75 \pm 0.81; \quad \text{or} \quad 17.94 < \text{days} < 19.56.$$

The number of days it took to develop scurvy should lie half and half within the limits 17.94 and 19.56. The actual division is 5 within and 3 outside—pretty good. The largest deviation is 2.25 which is less than thrice the probable deviation, and although a deviation so large will occur according to the Gaussian law in the long run only once in 16 times (instead of 8), this need not disturb us.

Having calculated the standard deviation σ we may use the formulas applicable to the Gaussian distribution and write, if n be the number of observations (here 8),

$$E_M = \text{probable error in the mean} = 0.6745\,\frac{\sigma}{\sqrt{n}}$$

$$= 0.6745\,\frac{\sigma}{\sqrt{8}} = 0.286$$

$$E\sigma = \text{probable error in the standard deviation}$$

$$= 0.6745\,\frac{\sigma}{\sqrt{2n}} = 0.202$$

The probable error in the mean is the probable error in the individual observation divided by the square root of the number of observations. This is a definition because it does not apply to all frequency laws, and as we shall see has no relation whatsoever to the experimental results now under examination. In like manner the probable error of the standard deviation σ is the probable error ($0.6745\,\sigma$) of the individual observations divided by the square root of double the number of observations. These results should be written as

$$\text{the mean } M = 18.75 \pm 0.29,$$
$$\text{the standard deviation } \sigma = 1.20 \pm 0.20.$$

Now we have another set of data obtained in another trial on boiled milk. Omitting the details of the calculations, the results for the mean, the standard deviation and the probable error in the mean are:

$$M' = 13.75, \quad \sigma' = 6.58, \quad E'_M = 1.29$$

The difference of the means in the two runs is

$$M - M' = 5.00 = 17 \times (0.29).$$

The second trial gives a mean 17 times as far removed from the mean in the first trial as the probable error in that mean. Shall we say that this positively could not have happened? Or shall we say that the statistical method positively can not be applied here as per rule? Even if we use the much larger error of 1.29 figured from the data in the second case, the difference $M - M' = 5.00 = 4 \times 1.29$, nearly, and this can happen less than once in 20 times—according to the rule, which we don't believe (Table I, second column, Gauss's Law).

I have taken the data on the time to develop scurvy when the milk is boiled. There are three sets on raw milk, with these values:

$$M_1 = 43.00 \pm 2.78,$$
$$M_2 = 20.71 \pm 1.36,$$
$$M_3 = 27.00 \pm 2.18.$$

Taking the largest possible error of 2.78 we find that

$$M_1 - M_2 = 22.29 = 8 \times 2.78,$$
$$M_1 - M_3 = 14.00 = 5 \times 2.78.$$

According to the rule there are 7 chances in 10,000 for M_3 to be so far from M_1 and an almost incalculably small chance for M_2 to be so far away. Those who know the laws of the multiplication of chances can perhaps figure what is the chance by rule that both M_2 and M_3 shall be so far from M_1. What is the conclusion? I say beware of probable errors or rather of the mere formal application of them to meager statistical material. Your conclusions will almost certainly be wrong. The sound conclusion from the fact that the means of the different runs fail to lie reasonably close together is that the data are statistically inconsistent or insignificant. Permit me to refrain from defining the coefficient of variability—we have already too many definitions.

I know only one thing about feeding experiments—namely, that I eat what I like when I please and that my weight has remained between 157 and 167 pounds since before I left Yale in 1907. That is scanty foundation for participation in this symposium. I do not wish to give the impression that the data submitted to me for criticism are valueless; for all I know they may show just what it was hoped they would show; perhaps the experiments only meant this for a preliminary study wherewith to get his bearings—such studies are often necessary. As statistical material the individual runs, of which there are seventeen, show by their mutual comparison (as illustrated above) that for some reason they are inadequate statistically. Is it that 7 or 8 or even 12 pigs are too small a sample? If the animals in the time required to develop scurvy were as homogeneous as galvanometer deflections under good conditions these numbers would show no such inadequacy. As probable errors, according to the rule, vary inversely with the square root of the number of animals, must we take 800 pigs in a series, hoping thereby to reduce the errors to one tenth of their present values? "Pigs is pigs" would then be our only hope.

Looking over the data as a whole I have come to certain tentative statistical conclusions. Out of 144 pigs 3 starved to death sooner than drink milk, and all but 6 others, that died in brief periods, developed

scurvy in from 8 to 58 days on a diet of milk, whether processed or raw. I judge that guinea pigs were not made for a milk diet. If one could find laboratory animals which would live to a ripe old age on raw milk but go scurvy on treated milk he would, I should think, have a more desirable stock for this experiment. How about that very popular laboratory animal—Drosophila? Would the white rat do? One may be venturing too far afield in using guinea pigs. My choice would be Drosophila if possible. And why? Because the strains are pedigreed; they can be made to order, so to speak; the infinite variety of nature can be somewhat controlled in them. With a controlled pedigreed animal, mated brother to sister, and used in the F_1 generation only, one might hope to approach the narrow and reproducible experimental conditions of the physicist and chemist. In that recent and most interesting book which you have all just read, "The Biology of Death," by Raymond Pearl, you can even find a life table for Drosophila and thus make something of an allowance for natural death that might otherwise complicate lethal experiments on feeding. There is a cumulative value to Drosophila.

However, it may be possible to use guinea pigs despite their obvious aversion to milk. But in their use we should try to attain as great genetic or constitutional homogeneity as possible. First as to weights. The 144 pigs under consideration varied in weight from 420 to 132 grams. This is all too much variation. It may conceivably not be important what the weight is, and yet unless the indifference of weight has been clearly demonstrated we should restrict ourselves to pigs of approximately the same weight. Second, there is the matter of age. I have no idea what the variability of ages was nor whether it has been shown that age is a matter of indifference in its effect on the time required to develop scurvy. Still, it is safe to strive for homogeneity in the material, to reduce so far as may be the number of variables. Now as to weight I have done one very simple thing. I have taken all the pigs fed on raw milk except the one that would not drink it; there were 23 of them. I made a chart by plotting "days to scurvy" vertically and "weight at the start" horizontally—and lo! a strong correlation stared me in the face (Fig. 2).

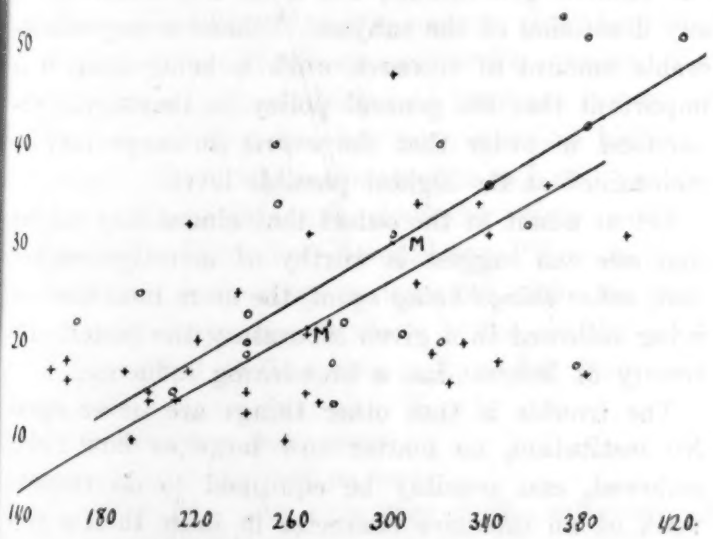

FIG. 2. A correlation diagram showing the tendency of the number of days (required to develop scurvy) to vary with the weight of the guinea pig at the start of the experiment. The abscissas (140–420) are the initial weights; the ordinates (10–50) are the number of days in which scurvy developed on a milk diet. Each circle represents these data for a single pig fed on raw milk and the upper line gives the general trend of these circles, M being the mean point. Each cross represents the data for a single pig fed on both pasteurized milk and the lower line gives their trend, M′ being the mean point. One may not compare the vertical distance between the means M and M′ without making allowance for the horizontal displacement between them.

It would take too long to define a correlation coefficient. In the "Biology of Death," pages 168–169, Pearl gives the definition in simple language for the intelligent οἱ πολλοῖ who frequent the Lowell Institute lectures. Suffice it here to state that the coefficient I found is $r = 0.68 \pm .07$. This is a high correlation and a small error. If I dared imitate Pearl on page 169 I should say that, the ratio of the probable error 0.07 to the coefficient 0.68 being over 9, the odds against such a correlation having arisen from chance alone are about 350,000,000 to 1; but I am too timid to follow him so far. It is enough for my purposes to note that the correlation is high and well established. I will not weary you with lines of regression or lines of closest fit, I have not calculated them. The diagram shows at a glance that for pigs fed raw milk, judging by this sample of 23, an addition of 100 grams in weight prolongs the time to develop scurvy by something like 15 days. This shows that the pigs should be uniform in weight or that the experimental data should be corrected for weight. It is not inconceivable to me that had I the ages of the pigs I should run into another correlation.

All the data for the 31 pigs fed bottle pasteurized milk were plotted up in like manner. The spread of points was again indicative of a sensible correlation coefficient, but I have not computed it nor estimated the correction for weight (except as one may do so roughly by glancing over the plotted points and guessing at a trend line). This should be done for each kind of treatment of the milk. One thing more I did. I figured the mean for raw milk and its probable error from the 23 available pigs and that for bottle pasteurized milk from the 31. The results were:

Raw $M_R = 30.65 \pm 2.4$ days. Avg. wt. $= 299$.
Bot. Past. $M_B = 21.4 \pm 1.0$ days. Avg. wt. $= 264$.

Without any correction for weight, the difference in the means, $M_R - M_B$, is 9.2, which is less than 4 times the probable error of M_R and as such would be just barely significant statistically when one considered the heterogeneity of the data and the great changes from sample to sample. An allowance for the difference of 35 grams in the average weights might easily reduce M_R to around 25 or 26 days and unless the probable error shrunk to less than 1.2 would eliminate whatever there was of statistical significance. If the probable error of M_B were used the result would be more hopeful, but in strict fairness we should use the formula for the probable error of the difference 9.2 of the two means as a function of their indiivdual probable errors; this would give a substantial indication of a significant statistical difference between the means provided, only no allowance for the weight correction be made.

We may raise the question of the comparison of the raw milk on the one hand and of the totality of treated milks on the other. I have not had time to compute the probable errors. The results for the means are:

				Reduced
Raw	M = 30.65 days	Avg. Wt. = 299		Raw
Bot. Past.	M = 21.4 "	"	" = 264	26
Boiled	M = 18.7 "	"	" = 238	22
Vat Past.	M = 18.2 "	"	" = 252	23
Autoclaved	M = 17.4 "	"	" = 232	22
Air Free	M = 15.5 "	"	" = 237	22

The last column contains a rough estimate of the value of the mean for guinea pigs of the given average weights if fed raw milk. It is my judgment, though I can give no numerical estimate of the probability of that judgment, that the following conclusions are reasonably safe:

(1) On treated milk the pigs do develop scurvy sooner than on raw milk even when allowance for weight is made.

(2) The difference in the time required is smaller than I should have expected.

(3) There is no indication that the different ways of treating the milk produce statistically different results.

(4) An experiment simultaneously performed with sets of 25 pigs of like age, weight and sex and as homogeneous genetically as possible would probably give a good deal of significant statistical data. (The size of the litters from which the pigs were taken might have to be kept constant.)

In bringing this paper to a close I must plead the brevity of my time for preparation as an excuse for so inadequate a treatment of the large amount of data submitted to me and of similar data found in the literature on feeding experiments. Statistical work carefully done takes time—not merely time for routine calculations but far more time for thought. I am glad to know that statistical studies are arresting the attention of biochemists. The physicists and engineers of the Western Electric Company have found that they must resort to such methods when dealing with measurements of such inherently variable phenomena as the microphonic properties of carbon as used in telephone transmitters where the utmost care does not suffice to control the properties to the extent ordinarily attainable in physics. And as the use of the statistical method spreads, we must and shall appreciate the fact that it, like other methods, is not a substitute for but a humble aid to the formation of a scientific judgment. Only with this philosophy in mind may we truly hope, with care, to avoid, in the main, being classed in the superlative category of that oft-cited sequence of liars, damned liars and statisticians!

EDWIN BIDWELL WILSON
HARVARD SCHOOL OF PUBLIC HEALTH

THE SELECTION OF SUBJECTS FOR RESEARCH

THE question whether students should select subjects for research entirely of their own choice, or from a list of subjects proposed by their chosen professor, has been raised in many places and by numerous student generations, but I do not recall seeing any discussion of the subject. Wherever any considerable amount of research work is being done, it is important that the general policy be thoroughly understood in order that the *esprit de corps* may be maintained at the highest possible level.

Let us admit at the outset that almost any subject that one can suggest is worthy of investigation and that, *other things being equal,* the more lines that are being followed in a given laboratory the better. Diversity of interest has a broadening influence.

The trouble is that other things are never equal. No institution, no matter how large or how richly endowed, can possibly be equipped to do research work of an intensive character in more than a very few fields in which students may profess an interest. It is not too much to say that any institution which attempted to offer research facilities to meet the supposed needs of every student would descend to superficiality. It would receive for its pains the contempt of its graduates and the neglect of the public.

On the other hand, no institution is so small or poor that it can not do something to increase the sum of human knowledge, provided that it adheres unswervingly to a sufficiently narrow program, mapped out perhaps many years in advance of its possible realization. Such a program furnishes its own justification. Only one criterion must be met. Does the

proposed program deal with fundamental problems or are the problems of only ephemeral interest? If the results will not be worth publication, the work is not worth attempting. If, on the other hand, the work is so very important to some particular industry that its results can not be published, the work should be done outside of the university laboratory.

As a matter of fact, training in research in our colleges and universities is really training in the methods of research and practically never in the particular research work which the student may care to follow later. The student takes up a problem suggested by his professor and according to the ethics of the matter which have gradually evolved, the student does not feel at liberty to continue the investigation of the same subject without the approval of the professor who first suggested the problem. The reason for this is obvious. Of the large number of students who have toiled so unceasingly in the Harvard laboratory in measuring the atomic weights with an accuracy heretofore unknown, how many have continued to measure atomic weights as part of their life's work? Bear in mind that I am not suggesting that the work going on in the Harvard laboratory is unimportant. It is most important and it had given that laboratory an international reputation, even before its unexpected fruition in the discovery of the *isotopes*. I do not even suggest that atomic weight work is not excellent training for chemists of the proper caliber.

The point which I wish to make is that the particular problem is of little moment to the student. He is learning research methods and they are not so very different, whether in chemistry, physics, biology or history. The work is monotonous and often discouraging. There is much chaff for every kernel of grain. To the student I would say, "Here is the test of tests for your absolute integrity, your tenacity of purpose, your ability to stand in the No-Man's Land of knowledge and profess a love for truth where there seems to be no truth. Here is

> Machinery just meant
> To give thy soul its bent,
> Try thee and turn thee forth, sufficiently impressed.''

Students are going to the Cavendish Laboratory in Cambridge, England, because Rutherford is working on radioactivity there; others are going to Cambridge, Massachusets, because Richards and Baxter are measuring atomic weights and volumes. Narrow fields for great laboratories, you say? Careful, you may not know how vast those little atoms are or how great their powers as shown by radioactivity. To-day is narrow only because it is here and now. Lift your eyes and to-day becomes a part of all time as broad as your mind can conceive. The narrow problem is like the

> Flower in the crannied wall,
> I pluck you out of the crannies,
> I hold you here, root and all, in my hand,
> Little flower—but if I could understand
> What you are, root and all, and all in all,
> I should know what God and man is.

Our tiny problem quickly branches out into more fields than any one mind can compass, and there must then come the process of delimitation.

If I may be pardoned a personal reference, I have tried to limit my own researches to the field of a certain kind of flow of matter. I say a "certain kind," because the subject is so large that I have had to exclude the ordinary flow which we have in rivers and pipes, yet the problems lead one through chemistry, physics, engineering, mathematics, biology, geology, not to mention excursions into the modern and ancient languages and the fascinating subject of philology. How is a student to be expected to have any interest in, let us say, plasticity, about which one knows scarcely anything as yet. But, if "everything flows," as Heraclitus maintained, who can say that it may not prove to be worth while to measure the plasticity of the rocks and metals as well as of rubber and paint? If it should turn out that the new knowledge was worth while, there would be a demand for men who possessed it, and students would be drawn towards this particular branch of knowledge. As a corollary, it is supposed that industries will seek elsewhere men with other types of specialized knowledge, and students desiring those types of knowledge will seek them elsewhere. There is thus a very desirable specialization going on in the universities of the world.

After all, the important thing is to be engaged on some problem. As Carlyle has said:

> Produce! Produce! Were it but the pitifullest infinitesimal fraction of a product, produce it in God's name! 'Tis the utmost thou hast in thee: out with it, then.

And whether we ourselves can produce or not, it little behooves us to harshly criticize the meager results of those who struggle on in the quest. Arthur Gordon Webster was a world authority on sound, the inspiration of his colleagues in physics for a generation, the life of the meetings of the American Physical Society, of which he was a founder. He imagined that his colleagues were no longer giving him their praise and encouragement, and he stopped his researches forever with a revolver shot. For lack of encouragement, others have stopped their researches at all periods of life or before really beginning them —not so dramatically as poor Webster, but quite as effectively. America needs to turn out a far greater volume of scientific research, and the only thing that is lacking is adequate popular support. In the selec-

tion of subjects for research we may safely follow the successful European practice, since only in this way may the morale be maintained.

EUGENE C. BINGHAM

LAFAYETTE COLLEGE

SCIENTIFIC EVENTS

THE BRITISH JOURNAL OF EXPERIMENTAL BIOLOGY

HITHERTO there has existed in Great Britain no journal which served specifically for the publication of researches in experimental biology lying outside the confines of genetics on the one hand, and traditional human physiology on the other. American workers who have created a powerful impetus to experimental inquiry in biological science will, it is hoped, welcome the announcement that a *British Journal of Experimental Biology* will appear in September, 1923, issued by Messrs. Oliver and Boyd, from the Animal Breeding Research Department at Edinburgh. While a primary object of the journal will be to promote in Great Britain the extension of inquiry along experimental lines, it is the earnest hope of the editorial board that American and continental scientists will give their support not only by subscribing but also by offering contributions for publication. All communications should be addressed to the Managing Editor, the Animal Breeding Research Department, the University, Edinburgh, Scotland.

F. A. E. CREW,
W. J. DAKIN,
J. HESLOP HARRISON,
LANCELOT T. HOGBEN,
JULIAN S. HUXLEY,
J. JOHNSTON,
F. H. A. MARSHALL,
GUY C. ROBSON,
A. M. CARR SAUNDERS,
J. MACLEAN THOMPSON.

FELLOWSHIPS IN MEDICINE[1]

"THE Rockefeller Foundation, New York, has entrusted the Medical Research Council with a fund to be used in providing fellowships in medicine in the United States. Fellowships will be awarded by the council, in accordance with the desire of the foundation, to graduates who have had some training in research work in the primary sciences of medicine or in clinical medicine or surgery and are likely to profit by a period of work at a university or other chosen center in the United States before taking up positions for higher teaching or research in the United Kingdom. A fellowship will be of the value of not less than £315 a year for a single fellow, or £470 for a married fellow, payable monthly in advance. Traveling expenses and some other allowances will be made in addition. A fellowship will be tenable for one year, which will as a rule begin in September. Applications for fellowships tenable for the academic years 1923–24 should be made not later than July 20th next. Full particulars and forms of application are obtainable from the Secretary, Medical Research Council, 15, York Buildings, Adelphi, London, W.C.2. It is understood that similar medical fellowships provided by the Rockefeller Foundation will be awarded by the National Research Council at Washington to American graduates desiring to work for a time at selected centers of research work in this country. Both announcements are of great interest. It is of course a commonplace to say that science is international and knows no boundaries; but the practical application of the principle frequently encounters difficulties, to the detriment of progress. Some of these difficulties are removed when scientific workers know those of other countries and their methods of work. The United States now possesses many first-rate laboratories and research institutes, and it will be a great advantage to the British fellows to work in them."

[1] From the *British Medical Journal.*

THE INFLUENCE OF MODERN SCIENCE ON HISTORY AND CIVILIZATION

DR. EDWIN E. SLOSSON, director of "Science Service," Washington, D. C., delivered a series of five lectures before teachers attending the summer session in the University of Pittsburgh. Schedule of these lectures follows:

July 16, "Gasoline";
July 17, "Refrigeration";
July 18, "Photography";
July 19, "Sugar";
July 20, "Coal-tar Products."

These lectures clearly illustrated the possibility of bringing to the layman a realization of his debt to science. Not only did Dr. Slosson show how we were indebted for conveniences, but he intimated how gasoline and other modern fuels had a tendency to spread civilization toward the poles, while the application of the principles of refrigeration made it possible to advance into tropical climates. In his talk on coal-tar products, he referred to Bayer 205, which Germany offered for her lost African colonies. He told how the discovery of a single chemical product might render it a medium of exchange in international relations.

In the talk on photography, applications of the four dimensions and Einstein's principle of relativity in our every-day motion picture were cited.

Dr. Slosson is to be commended for the compelling evidence which he has gathered to show that science is

not only a matter of interest in the laboratory and the factory, but that it is of vital importance to men in every-day life.

A. SILVERMAN

EXPLORATIONS FOR RUBBER

The Journal of Industrial and Engineering Chemistry gives the following particulars in regard to expeditions now investigating possible sources of rubber supply:

W. L. Schurz, United States commercial attaché to Brazil, is in charge of the field expedition that is to investigate the Amazon rubber region in behalf of the Department of Commerce. He will be assisted in this work by O. D. Hargis, rubber plantation expert, and C. F. Marbut, of the Bureau of Soils, Department of Agriculture, who will make a study of the soils of this region in reference to rubber production. This party will cooperate with four experts from the Bureau of Plant Industry—Carl D. La Rue, James R. Weir, E. L. Prizer and M. K. Jessup—whom the Department of Agriculture has sent to Brazil to make a biological study of rubber plants in the Amazon Valley. They sailed from New York direct for Para about the middle of July and will probably be gone about eight months.

D. M. Figart, a special agent of the Department of Commerce who is well known in Far Eastern circles, sailed from the United States last month on his way to southern India, Ceylon, British Malaya and the Dutch East Indies, where he will make a comprehensive study of all phases of the rubber industry.

Two other parties, one for northern South America, Central America and Mexico, and one for the Philippines, are being planned.

Several months ago Harry N. Whitford, professor of tropical forestry, Yale University, was appointed by the Department of Commerce and placed in charge of the investigation of sources of crude rubber and the possibilities of developing rubber plantations in the Philippine Islands and in Latin America. J. J. Blandin, formerly in charge of rubber plantations of the Goodyear Tire and Rubber Company, was designated as his assistant.

SCIENTIFIC WORK IN SIBERIA

PROFESSOR T. D. A. COCKERELL, of the University of Colorado, in a letter from Vladivostok, Siberia, says:

We arrived here safely on July 3 and had no difficulty whatever with the authorities. We had, however, been detained a week in Tsuruga, Japan, because the Osaka Shosow Kaisha would not take us on their ship, fearing trouble with the Russians at this end. Next Tuesday we hope to start for the Amagu River (about 400 miles up the coast), to examine the deposit that contains fossil insects. We go in a small trading steamer. Last night we were entertained at dinner by a company of scientific people of Vladivostok, and speeches were made by Mr. N. Solovieff, director of the museum, and Mr. Davidoff, director of the Hydrographical Institute, both expressing the most cordial attitude toward American scientific men, and the desire for better understanding and more cooperation. We find a very considerable amount of important scientific work going on here. Thus the geographical committee, under Dr. Kryshtofovich, has issued a series of very interesting contributions to geology and paleontology; Dr. Paul de Wittenbourg, with whom we have become well acquainted, has published extensively on local geology; Dr. Arnold Moltrecht, an exceedingly keen naturalist of wide experience, has done a great deal of work on the Lepidoptera; the Hydrographical Survey is actively preparing and publishing review maps of the coast, the old maps being in some cases at least 20 miles wrong, resulting in shipwrecks. They are also collecting quantities of marine animals. Mr. Vladimirsky of the observatory is keeping the most accurate records, using in some cases new instruments which he has designed or invented, and had made in Vladivostok. The Commercial School, under Mr. Lutrenko, is a large and excellently equipped institution, with departments of biology, chemistry, physics, etc. Mrs. J. K. Shishkin is a botanist who has worked on the flora of the region, and has prepared a good herbarium, the condition of the plants being exceptionally fine. Altogether, we find much scientific activity, in spite of many unfavorable conditions, and every promise of important developments in a country extraordinarily rich as a field for research. All the men desire cooperation with the scientific men of America and are extremely anxious to obtain our more recent publications, especially those of a general character, monographs, and works on the natural history of the Northern Pacific countries. We expect to sail from Yokohama for Vancouver about September 1, but it all depends on when we can get back from the Amagu River.

APPOINTMENTS IN AGRICULTURE AT THE UNIVERSITY OF CALIFORNIA

TRACY I. STORER, field naturalist of the California Museum of Vertebrate Zoology, University of California, has been appointed assistant professor of zoology at the Branch of the College of Agriculture at Davis.

William Adams Lippincott, professor of poultry husbandry in the Kansas State Agriculture College, has been appointed professor of poultry husbandry in the university and head of the department of poultry husbandry at the Branch of the College of Agriculture at Davis.

William Henry Chandler, professor of pomology and vice-director of research of the New York State College of Agriculture, Cornell University, has been appointed professor of pomology in the University of

California to have charge of the work in pomology at Berkeley.

With the appointment of Harry S. Smith, chief of the Bureau of Pest Control, Sacramento, as associate professor of entomology in the University of California, the work in the biological control of insect pests formerly conducted by the State Department of Agriculture has been transferred to the University of California. Mr. Smith assumed his duties in connection with the university on July 1, and is stationed at the Citrus Experiment Station, Riverside. H. M. Armitage, entomologist, and A. J. Basinger and Harold Compere, assistant entomologists, will be stationed at the Whittier Laboratory which is to be retained for this work. The regulatory and police functions of the Bureau of Pest Control will be retained by the State Department of Agriculture. The university will continue to maintain one or more explorers in foreign countries in the search for promising beneficial insects. Mr. E. W. Rust, the present parasite collector, will be stationed in South Africa during the coming year.

SCIENTIFIC NOTES AND NEWS

DR. T. G. BONNEY, the distinguished English geologist, celebrated his ninetieth birthday on July 27.

IN connection with the recent international conference of chemists in Cambridge the doctorate of science was conferred upon Dr. Wilder D. Bancroft, professor of physical chemistry at Cornell University.

IN addition to the election of Professor Charles F. Chandler to honorary membership in the Society of Chemical Industry, announced last week in SCIENCE, Prince Ginori Conti, president of the Italian Chemical Society; M. Paul Kestner, president of the French Society of Chemical Industry; Professor Joji Sakurai, Japan, and Sir Dorabji J. Tata, India, have been elected honorary members.

M. JEAN PERRIN, professor of physical chemistry at the Sorbonne, has been elected a member of the Paris Academy of Sciences, in place of M. Bonty. M. René Maire has been elected correspondent in botany to succeed the late M. Battandier, and Sir Robert Hadfield, of Sheffield, correspondent in chemistry in the place of M. Paterno, who has become associate member.

IT is announced in *Nature* that the Royal Danish Academy at its last annual meeting elected the following honorary foreign members: Professor Albert v. Le Coq, of Berlin, Professors Charlier, J. Forssman and C. M. First, of Lund, Dr. F. A. Bather, of the British Museum, and Professor F. O. Bower, of Glasgow.

THE staff of the laboratory of the Henry Phipps Institute of the University of Pennsylvania recently gave a farewell dinner in honor of Dr. Paul A. Lewis, for thirteen years director of the laboratory. Dr. Lewis has accepted a position at the Rockefeller Institute, Princeton, N. J.

THE title of emeritus professor has been conferred by the University of London on Dr. W. D. Halliburton, on his retirement from the chair of physiology at King's College, which he has held since 1890.

PROFESSOR ANDREW GRAY is retiring from the chair of natural philosophy of the University of Glasgow in which he succeeded Lord Kelvin in 1899.

PROFESSOR CHRISTIAN EIJKMAN, of Utrecht, is retiring from his chair and the charge of the Hygiene Institute, having reached the age limit. His pioneer work on beriberi and other deficiency diseases was done in the Dutch East Indies, but he continued to work after returning to the Netherlands in 1898.

DR. ERNST BECKMANN, formerly professor of applied chemistry at Leipzig and until recently director of the Institute for Chemistry at Dahlem, celebrated his seventieth birthday on July 4.

THE Triennial Congress of the International Surgical Society, under the presidency of Sir William Macewen, was formally opened on July 17 by the Prince of Wales at the Royal Society of Medicine, London, before a large gathering of delegates, representing about sixteen nations. Lord Curzon of Kedleston, secretary for foreign affairs, and Mr. Neville Chamberlain, minister of health, welcomed the delegates on behalf of the government. In his address of welcome the Prince of Wales said: "I am also glad to extend that welcome to your last president, Professor Keen, of Philadelphia, who, in spite of the eighty-six years he carries so lightly, and the weight of a world-wide reputation, is again amongst us ready, as always, to take part in every meeting—social as well as scientific—of this distingiushed gathering."

A PORTRAIT of President James Rowland Angell, of Yale University, formerly professor of psychology and dean of the faculties at the University of Chicago, has recently been completed by Ralph Clarkson, of Chicago, who has painted portraits of a number of other members of the faculties, including Professor Albert A. Michelson, head of the department of physics; Professor Thomas C. Chamberlin, formerly head of the department of geology, and Professor Rollin D. Salisbury, late dean of the Ogden Graduate School of Science. A portrait of Professor John Merle Coulter, for twenty-seven years head of the department of

botany, has been painted by a well-known artist from Vienna, and will be presented to the university by colleagues and former students of Professor Coulter.

SIR JOHN BLAND SUTTON has been elected president and Sir Berkeley Moynihan and Mr. H. J. Waring have been elected vice-presidents of the Royal College of Surgeons of England.

AT the annual meeting of the Manchester Literary and Philosophical Society, Professor H. B. Dixon was elected president; the vice-presidents are Mr. T. A. Coward, Professor A. Lapworth, Mr. C. E. Stromeyer and Professor F. E. Weiss.

AT the annual general meeting of the Röntgen Society, London, the following officers were elected: *President,* Sir Oliver J. Lodge; *vice-presidents,* Sir Ernest Rutherford, Dr. A. E. Barclay and Dr. F. W. Aston.

SIR FRANK DYSON has been appointed to represent the International Astronomical Union on the International Research Council. Norway, Spain, Switzerland and Portugal have now joined the union.

MR. E. G. BOULENGER, at present curator of reptiles at the Zoological Gardens, London, has been appointed director of the new aquarium. Miss Joan B. Procter has been appointed curator of reptiles.

DR. WALLACE CRAIG, formerly professor of philosophy in the University of Maine, has been appointed librarian in the department of biophysics of the Cancer Commission of Harvard University.

MR. IVAR N. HULTMAN has recently been made chemist in charge of operations at the Kingsport plant of the Tennessee Eastman Corporation. At the close of the war Mr. Hultman became associated with the synthetic organic chemistry department of the Eastman Kodak Company, at Rochester, N. Y. About two years ago he was transferred as chemist at the Passaic plant of the Eastman Chemical Corporation, which position he has held until the present time.

THE following have recently joined the research laboratory staff of the Eastman Kodak Company: Robert F. Mehl, who finished his graduate work at Princeton this year; Paul A. Anderson, who finished his graduate work at Harvard this year; Dr. Merle Dundon, National Research Council Fellow at Ohio State during the past year; Arthur B. Corey, instructor in chemistry, University of Vermont, during the past two years; J. L. Miles, Dartmouth, 1923; E. H. Grupe, Union, 1923; George W. Chapin, Allegheny; Harold C. Folts, Syracuse, 1923, and E. M. Lowry, instructor in physics, Syracuse University, during the last two years.

THE annual representative meeting of the British Medical Association opened at Portsmouth on July 20 under the chairmanship of Dr. R. Wallace Henry, of Leicester, clerk of the representative body. Mr. J. Basil Hall, M.A., F.R.C.S.E., was elected president of the association for 1924–25, and it was decided that the annual meetings in 1925 should be held in Bath. Sir Daniel Drummond, president of the association in 1921–22, was chosen a vice-president in recognition of his past services, and the following were elected overseas vice-presidents of the association: Mr. George Adlington Syme (Melbourne), Dr. Robert Henry Todd (Sydney), Dr. Harry Edward Gibb (Wellington, New Zealand), Dr. Wilfred Watkins Pitchford (Johannesburg), Dr. Dugald Campbell Watt (Pietermaritzburg). Dr. S. Morton MacKenzie (Dorking), chairman of the organization committee, in presenting that body's report, said that the association now had 25,454 members. Two thousand new members had joined since the beginning of the year. Allowing for losses from death and other causes there had been a gain of 1,172 in the membership since January last. Of their members 607 had belonged to the association for more than forty years.

THE 104th annual meeting of the Swiss Society for Natural Sciences will be held from August 30 to September 2 at Zermatt. This will be the fifth occasion when the society has met in the Canton of Valais. The work of the meeting will be divided into fifteen sections as follows: (1) Mathematics, (2) physics, (3) geophysics, meteorology and astronomy, (4) chemistry, (5) geology, mineralogy and petrography, (6) botany, (7) zoology, (8) entomology, (9) paleontology, (10) anthropology and ethnology, (11) medical sciences, (12) history of medicine and the natural sciences, (13) veterinary medicine, (14) pharmacy, and (15) engineering science. In addition to the sectional gatherings, there will be general discussions which will be addressed by distinguished men of science. Among the topics thus dealt with will be: Phylloxera in Valais, by Dr. H. Faés, director of the Federal Viticultural Station, Lausanne; earthquakes in Switzerland, by Dr. A. de Quervain, of the University of Zurich; and the geology of the neighborhood of Zermatt, by Professor E. Argand, professor of geology, paleontology and petrography in the University of Neuchâtel. The following officers have been appointed for the meeting: *President,* Rev. C. M. Besse; *vice-president,* Dr. J. Amann; *treasurer,* M. E. de Riedmatten, and *secretary,* M. A. de Werra, of Sion, Valais.

THE agricultural and food division of the American Chemical Society combines with the fertilizer division at the Milwaukee meeting of the society in September to hold a symposium on fertilizers, soils and crops.

In the past few years there has seemed to be a need for what may be termed the problem viewpoint of research in soil problems. The different divisions of work are so woven together that it takes a meeting bringing together men from the different branches of science, pure and applied, to adequately discuss the problems involved. We are requested to state that those who can present papers on subjects that should come before this meeting as well as those who have subjects or speakers to suggest should communicate with H. A. Noyes, chairman of the agricultural and food division of the American Chemical Society, in care of the New Rochelle Research Laboratories, New Rochelle, New York.

The one hundred and twenty-eighth meeting of the American Institute of Mining and Metallurgical Engineers, it is announced, will be held on August 20 to 31 in Ontario and Quebec. With the cooperation of the Minister of Mines of these two provinces and of the members of the Canadian and American Institutes of Mining and Metallurgical Engineers, the vast mineral wealth of Canada will be placed on exhibition. Petroleum and gas will be among the principal general subjects of the technical sessions, which are to be held in Montreal. The geology of Santa Elena oil fields, Ecuador, will be discussed by Joseph H. Sinclair, of New York, and Professor Charles P. Berkey, of Columbia University. Other papers will be presented by A. F. Meston, of New York, and Oliver U. Bradley, of Muskogee, Okla.

Earl G. Sturdevant, who has served as professor of physical chemistry at Western University, London, Canada, since 1920, resigned this position on June 1 to accept a position with the Western Electric Co. as development engineer. Dr. Sturdevant is working on the manufacture of insulated wire.

John H. Schmidt has resigned his instructorship in organic chemistry at the University of Wisconsin to accept a position as research chemist with the Redmanol Chemical Products Co., Chicago, Ill.

W. C. Gangloff, formerly research chemist with the National Aniline and Chemical Co., is now chief chemist with Dextro Products, Inc., Buffalo, N. Y.

The public health service of Spain has appointed the following three physicians to the fellowships offered by the Rockefeller Foundation for a visit of inspection and study in America: Drs. Fernández Besones, inspector for Salamanca province; Ortiz de Landazuri, of the central sanitary brigade, and Fuegó, the bacteriologist of the public health headquarters at Vigo.

Dr. C. E. Kenneth Mees, director of the research laboratory of the Eastman Kodak Company, sailed for England on July 6 for an extended business trip. Dr. S. E. Sheppard has been made acting director of the laboratory.

Professor Ralph G. Wright, head of the department of chemistry of Rutgers College and the State University of New Jersey, has been granted a year's leave of absence. Ernest Little, acting professor of chemistry at the university, will be the acting head of the chemistry department during Dr. Wright's absence.

Leave of absence has been granted to Professor Cossar Ewart, Regius professor of natural history in the University of Edinburgh, to enable him to accept the invitation of the government of Australia to attend the Pan-Pacific Science Congress in Australia. His recent researches into the origin of the merino sheep and on the evolution of wool have been of great practical value, and he has been specially invited to contribute to the proceedings of the congress.

UNIVERSITY AND EDUCATIONAL NOTES

The student body, alumni and friends have decided to establish a research endowment foundation at the Detroit College of Medicine and Surgery. It is hoped to raise at least $100,000 to make the foundation permanent. It is announced that a scholarship in pathology, valued at about $18,000, has been raised during the past year, and that the alumni association is now engaged in raising a European scholarship of $20,000, which will enable the college to send at least one student to Vienna, Glasgow or Edinburgh each year.

Following on the death of his widow, the estate of the late Professor Campbell Brown has been handed over to the University of Liverpool under the conditions stated in his will. These provide that: (1) A Campbell Brown chair of industrial chemistry shall be established, the first professor to specialize in oils, fats and waxes. In the first instance the salary shall be £1,000 per annum. (2) The income of a sum of £5,000 shall be placed at the disposal of the professor for the upkeep of his department. (3) A Campbell Brown fellowship, value £150 per annum, and (4) The balance to be used for scholarships.

With a view to improving the health conditions of the Japanese people, Baron Masuda and Baron Mitsui have each contributed 200,000 yen to the Keio University to establish a food research laboratory in connection with the university hospital. This food laboratory will be the first of its kind in Japan. Dr. Kenta Omori, the head of the research work, and Dr. Z. Kawakami will soon be sent by the Keio University authorities to America and then to Europe for the

purpose of studying similar undertakings in those countries.

PROMOTIONS to associate professorships in the faculties of the University of Chicago are announced as follows: Georges Van Biesbroeck and Storrs B. Barrett, in astronomy; Edward S. Robinson and Forrest A. Kingsbury, in psychology; Arthur J. Dempster, in physics; Warder C. Allee, in zoology; Esmond R. Long, in pathology; Emery T. Filbey and Guy T. Buswell, in the College of Education.

MR. G. C. STEWARD, fellow of Gonville and Caius College, has been appointed fellow and lecturer in mathematics at Emmanuel College, Cambridge.

DISCUSSION AND CORRESPONDENCE

PROPOSALS FOR THE PRESERVATION OF THE WISENT

IT seems to be unfortunately only too true, occasional contrary reports notwithstanding, that the European bison or wisent (Bos bonasus L., Bison europaeus Ow.) of which prior to the great war about 700 lived in the Lithuanian forest of Bjeloviesh, near Grodno, has been exterminated there. The fate of the wisents that lived in northwestern Caucasia is unknown, but in all probability this herd no longer exists and of the herd formerly living in the estates of Prince Pless, Upper Silesia, only three remain. Thus, when summed up, no herd exists any longer, only some fifty head, which are scattered widely in zoological gardens or preserves.

Dr. Kurt Priemel, director of the Municipal Zoological Garden in Frankfurt and an expert on the matter, suggests that by a systematic cooperation of all persons interested the wisent may be permanently saved or at least preserved for many a year to come. Dr. Priemel proposes that the methods so successfully pursued by the American Bison Society be applied to preserve the wisent. To do this, however, the cooperation of all interests, German and foreign, is indispensable; only thus can systematic breeding be carried out, the necessary funds raised and general interest for the plan gained. It is proposed, therefore, to found a Society for the Preservation of the Wisent. Statistical data have been made up and a card catalogue arranged in which information regarding all the known living wisents has been compiled. The most important problem for the new society, when formed, will be to endeavor to increase the number of animals by systematic breeding, an interchange of individuals from various sources being also considered. As it is, with one exception, all the available stock derive from the Bjeloviesh herd. This exception is a bull of the Caucasus breed, which has always played an important part in reproducing the race and will continue to do so, as long as possible. The animal was presented to Karl Hagenbeck in 1907 by the Czar.

Should systematic breeding produce satisfactory results, and successful experience of many zoological gardens tend to encourage this hope, in twelve to fifteen years small herds of wisents may be turned loose in extensive preserves. These preserves should offer, as far as possible, variegated topography, climate and forest formations. A beginning has been made by Count Arnim-Boitzenburg, of Boitzenburg-Uckermark, province of Brandenburg, who has introduced the wisent herd formerly owned by Hagenbeck upon his estates. It is to be hoped that the dangers of inbreeding may be obviated and a healthy development assured.

THEODOR G. AHRENS, PH.D.
STAATLICHE STELLE FÜR NATÜRDENKMALPFLEGE,
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THE AMERICAN EDUCATOR AND SCIENTIST

THE title-page of the recently issued "The American Educator and Scientist, a Vocational Blue Book," bears the slogan: "Representation a Criterion of Distinction." After glancing through the text one is tempted to wonder what is the criterion of representation. Among the many who are not "represented" are the president of Harvard University and the president of the National Academy of Sciences.

Nothing is specified regarding the life work of James Furman Kemp except that he is manager of the New York Botanical Garden. Charles Whitman Cross is given all the credit for authorship of Quantatative Classification of *Ingenious* Rocks, although in the next entry but one Whitman Cross is described as author of Quantitative Classification of *Ingenious* Rocks.

This work is one of twelve blue books issued annually by the same publishers. The series includes The American Elite and Sociologist. The price of each volume is $10.

OMISSUS
WASHINGTON, D. C.

SCIENTIFIC BOOKS

Grundzüge der Paläontologie, II Abt., Vertebrata. By K. A. VON ZITTEL. Neuarbeitet von F. Broili und Max Schlosser. R. Oldenbourg, München u. Berlin. 4e Auflage, 1923.

THE new edition of this classic text-book will be a great boon to all workers and teachers in vertebrate paleontology. There are unfortunately no compre-

hensive and up-to-date text-books in English on this subject, or at least none of this type. Lull's "Organic Evolution" is admirable of its kind; it is didactic, aiming to set forth and expound principles and to illustrate them by selected examples from the fossil record. Osborn's and Scott's well-known books cover their selected fields excellently and authoritatively, and various other recent English and American books are indispensable. Zittel's text-book, however, is of a different kind. It is rather a dictionary than a text-book in the usual sense. It is a systematic account of the fossil vertebrata known to science, with a concise summary of how much is known of each genus, its diagnosis, relationships and occurrence of the known species. Brief outlines of the essential distinctions of the larger groups, with very complete and well-selected lists of references for each group, a few final pages reviewing the record as a whole and an index of names bring the volume up to a little over 700 pages of large octavo. Save in the matter of taxonomy and with exception of casual comments here and there on phylogenetic relationships, the book avoids any statement of theories and principles and confines itself entirely to facts of record. It does not explain evolution or defend it; it does not discuss the causes of extinction or the reasons for the dominance of reptiles in the Mesozoic, or any of the thousand and one problems that the study of the record suggests to an inquiring mind. But it does give the facts, completely and accurately and conveniently, and whether you desire to identify a specimen or to look up an unfamiliar genus, to find out what is known of the geologic history of some group or to get an exact knowledge of the fossil evidence illustrating some principle or bearing on some problem to be set forth, you refer with confidence to your Zittel. The essentials of the known record will be there and the bibliography will enable you to run down details so far as available.

There are several other excellent German text-books in this subject, notably those of Abel and Strömer, but none of them quite on the Zittel plan. The school of paleontology which Zittel built up at Munich was preeminently a school of facts, of exact practical knowledge of the science, based on the study and comparison of the great collection of fossils brought together under his direction, and since his death in 1904 the same policy has been consistently and ably pursued under direction of Doctor Broili and his colleagues. The results are reflected in the magnificent collections of fossil vertebrates and invertebrates in the Munich Museum of Paleontology, selected and arranged for purposes of study and research, and in the revised editions of the Grundzüge issued in 1911, 1918 and 1923. The original edition was translated into English by the late Dr. C. R. Eastman in 1902; no translation of the revised editions has been published, chiefly because it has been difficult for American specialists to accept the taxonomic arrangements unchanged or to agree upon modifications to be adopted.

The present edition has been brought up to date by the inclusion of nearly all the new discoveries down to 1922; a few omissions here and there are mostly unimportant. Various changes have been made in the arrangement and in the descriptive text of the 1911 and 1918 editions; the chapter on dinosaurs has been extended and partly rewritten to conform to recent researches and discoveries; and many additions and minor changes appear throughout the volume. American researches and discoveries are well presented—as indeed they are in most of the German text-books on fossil vertebrates. Many new illustrations have been added; as in preceding editions, the illustrations are always of specimens, few diagrams and no restorations. The only exception is a restoration of the head of *Camarasaurus* (after Osborn) placed beside the skull and jaws on which it was based. A certain system is noticeable, especially with the fossil mammals, in the attempt to represent the skull, the dentition and the feet of the better known types, more rarely the complete or reconstructed skeleton, thus bringing out the salient distinctive characters of each group.

While the treatment is, on the whole, both complete and up to date, there are naturally various minor errors and omissions and a few points open to more serious criticism. The retention of *Lysorophus* among the lizards, although admittedly in a provisional way, appears hardly defensible in view of the researches of Williston, Case, Sollas and others, and its attribution to a family "Paterosauridæ," which is not a proper family name, as it is not based upon any described genus, is contrary to the rules of nomenclature. The association of *Diatryma* and *Phororhachos* is hardly tenable in spite of their remarkable convergence, but perhaps it was not intended to be other than provisional. The primary division of the Mammalia into Eplacentalia (Monotremes + Marsupials) and Placentalia, would hardly be approved by any anatomist. *Blastomeryx* is not a *Hypertragulid,* in spite of the fact that its lateral metacarpals are complete, but a true Pecoran, with the characteristic cannon-bone and other distinctive features of the higher ruminants. The omission of the Paleanodonta, primitive Edentates of the North American Eocene (*Paleanodon* and *Metacheiromys*) is probably owing to Doctor Schlosser's doubts as to

their real affinities (which the reviewer does not share). But they should have been placed somewhere, for they are important types, both known from skulls and skeletons, and upon no theory of their affinities is their evidence negligible. No reference is made to the gigantic and peculiar rhinoceroses first discovered by Cooper in Baluchistan, reported by Borissiak from Turkestan, and quite recently found in Mongolia by Andrews and Granger. There are unfortunately a good many misprints, and the quality of the paper is not up to the old pre-war standards.

But with allowance for all defects, which, after all, are very few or of quite minor importance in comparison to the enormous mass of facts stored within its covers, the authors deserve the cordial thanks and appreciation of all who are interested in fossil vertebrates, in their completion of a revision that involves an immense amount of labor and erudition and a comprehension of the essential facts of the discoveries they have summarized that is far from common, to judge from the misstatements and misunderstandings of the average critical review.

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QUOTATIONS

THE ANNUAL EXPOSITION OF CHEMICAL INDUSTRIES

THE Ninth Annual Exposition of Chemical Industries is to be held in New York City, Grand Central Palace, September 17 to 22, 1923. The exposition has always been a place where those with inquiring minds could learn much in a short time regarding chemical equipment and the chemical industry. In late years the junior chemical engineers at Yale have assembled under Professor Read to devote the mornings to serious study and the afternoons and evenings to gaining a more intimate acquaintance with chemical equipment on display. The Advisory Committee of the Exposition recommended that a special feature be made of these facilities this year, and an announcement has been issued offering students of chemistry and chemical engineering a course on the fundamentals of chemical engineering and industrial chemical practice. Lectures will be given by men prominent in the various specialties, and a committee of educators has undertaken to make the course as attractive and profitable as possible. Three principal topics are to be the centers about which the work will be done. These are:

1—Plant Equipment in the Chemical Engineering Industries.
- (a) Disintegration—Crushing and Grinding.
- (b) Mechanical Separation—Grading.
- (c) Separation of Solids from Liquids—Thickening, Filtration, Centrifugal Separation.
- (d) Separation with Phase Change—Evaporation, Distillation, Drying.
- (e) Handling of Materials.

2—Materials of Construction—What materials to use, when, where and why.

3—Chemicals in Commerce—The distribution of chemicals.

It is expected that no charge will be made to students. The Exposition management asks instructors to advise how many of their students will care to avail themselves of this opportunity, and further undertakes to assist students in securing living accommodations while in New York. The seriousness of the work is indicated by the announcement that a report or examination on some phases of the course may be required by the committee.

We urge chemists and chemical engineers, whether students in institutions of learning or otherwise, to take advantage of this unusual opportunity. The men most familiar with the equipment and unit processes will be present. Exceptional facilities for examining devices of different designs and makes will be offered. It will be possible to meet those interested in the same field of work and discuss problems with them. Here is an opportunity to gain in one short week information of value, comparable perhaps to the concentration of data commonly found only in hand-books. Do not miss it!—*The Journal of Industrial and Engineering Chemistry.*

SPECIAL ARTICLES

THE PRODUCTION OF "BROWN-SÉQUARD'S EPILEPSY" IN NORMAL NON-OPERATED GUINEA PIGS

BROWN-SÉQUARD was the first to report that operative insults to the nervous system, such as lateral hemisection of the cord, section of the dorsal columns, section of one or both sciatic nerves, produced after a certain lapse of time a variety of interesting motor disturbances in the guinea pig. These disturbances were characterized by attacks of complex, coordinated, tonic and clonic contractions of the muscles of the head, neck, trunk and legs. The motor discharges occurred spontaneously or as the result of pressure stimulation of a certain receptive field of the skin which Brown-Séquard called "epileptogenic zone." This area comprehended roughly the side of the face below the eye and extended backwards, including the scapular region. The zone was unilateral after unilateral lesions and was always located on the operated side when the cord was involved; after damage to the brain, however, the "epileptogenic zone" shifted to the opposite side. In addition to the manifestations mentioned Brown-Séquard also described a transient

"blepharospasm," a "bronchorrhoea" and the expulsion of feces, urine and semen during the attack. Brown-Séquard never observed the symptoms in normal guinea pigs, but Graham Brown[1] in his excellent analysis of these motor phenomena mentions briefly some suggestive motor reactions observable in the normal guinea pig; these appear to have been slight and inconstant, however.

The chief symptoms of the complex known as "Brown-Séquard's epilepsy" may be obtained in the normal, non-operated guinea pig provided certain conditions are fulfilled: (1) The animal must be accustomed to his surroundings, including the experimenter; (2) abrupt movements, unaccustomed noises and apparently also odors, sudden vibrations must be avoided; otherwise the typical reactions following tactile or pressure stimuli of the skin are largely or entirely inhibited. The length of time necessary to fulfill conditions varies considerably in different animals, especially when only touch stimuli are employed.

The touch stimuli were applied by means of long bristles mounted on handles; the pressure stimuli by means of long, slender, wooden rods one eighth to three sixteenths inch in diameter; the stimuli were both of short and long duration and were applied frequently.

The sites where symptoms are most easily elicitable are: (1) A small area just posterior to the angle of the jaw and below the ear; and (2) an area just anterior to the pelvis and above the femur when the animal is in a squatting position. Pressure stimuli are most effective, especially in the beginning. Either side of the body may be stimulated effectively.

Some of the reactions obtained after pressure stimuli in the lumbo-pelvic area are as follows: a lateral arching of the body, the nose of the animal approaching the site stimulated, with rotation of the head so that the plane of the lower jaw is turned towards the body. Associated with this arching, the hind leg of the side stimulated is usually flexed suddenly and strongly at hip and knee, the foot is dorsally flexed at the ankle while the toes may be extended and spread far apart, or the toes may be close together and slightly flexed. At the same time the hind leg of the opposite side is usually strongly and forcibly extended, raising the posterior half of the body, or it may be strongly abducted. The hind leg of the side stimulated may execute a series of scratching movements, the toes moving towards the jaw of the same side; at times the toes actually scratch the jaw and then the toes are usually spread apart. At other times the toes merely approach the jaw and then in general are not spread; when this occurs the dorsal lift of the femur is apparently especially marked, and the movement now resembles the thigh wiping-reflex of the frog. The scratch reaction may consist of from one to eight and perhaps more, exceedingly rapid flexions and extensions of the thigh, leg and foot. In animals which have been tested for months this pressure stimulus causes the lateral curvature of the body and neck, but often both hind legs are violently extended at the same time, so that the animal is hurled into the air; the animal, however, always righted itself in midair and was never observed to fall on its side or to roll over.

Pressure stimuli just posterior to the angle of the jaw usually cause, in addition to escape movements, a general shaking of the body similar to that executed by a wet dog, with more or less erection of the hair; wiping of the head and face with the front leg of the side stimulated, the head being rotated towards the non-stimulated side, or a more or less marked scratch reaction of the hind leg of the same side may occur. Often the front leg of the side stimulated may execute a series of rapid back and forth shaking movements.

Touch stimuli may give the same motor reactions, but in addition they permit the observation of other responses which are partially masked when pressure stimuli are applied; some of these are chewing motions, slow, audible, grinding movements of the teeth, dorsal or ventral nodding movements of the head, sneezing associated with unilateral or bilateral wiping of the nose, short convulsive contractions of the muscles of the trunk, neck and legs which raise the trunk slightly from the floor, the head being flexed ventrally. Associated with the latter movements one may observe at times a spasmodic retraction of the angles of the mouth accompanied by a wide separation of the jaws and strong closure of the eyelids.

Increased defecation, urination may be readily observed in these animals after repeated pressure or touch stimulation. In two instances several drops of a milky fluid suddenly issued from the nares (a naris?) and fell upon the reflecting mirror of the observation cage where the animal was being tested. The ejaculation of semen was never seen by me.

From this brief description it will be perceived that the so-called "incomplete attack" of Brown-Séquard may readily be obtained under proper conditions in normal guinea pigs. The absence of the "complete attack" is probably attributable to the normal muscular balance of these animals.

The material upon which this study is based consists of about 60 animals, chiefly males; all of them were under close daily observation for months.

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[1] T. Graham Brown, *Quart. Journ. Exp. Physiol.*, 1910, III, 144.